ON THE ORIGIN AND THE CAUSATION OF VITAL MOVEMENT.[1]

II.

TO this end permit me to go a little into detail concerning nerves. Nerves are processes of nerve-cells composed of fibrils of immeasurable fineness, which, in the so-called axis cylinder of the medullated nerves, are united by a stroma inside a very fine membrane called the axolemma. In proportion to the microscopic dimensions of the ganglion cells, of which the separate nerve-fibres form a part, these latter are for the most part enormously long, many as long as our arms and legs, and that is one of the reasons why the perception of the unicellular nature of the nerves made way but slowly. In fact, it was not easy to accustom oneself amid the microscopic swarm of cells, to find single ones so grown in length that they could be wound about us like a cocoon thread. As it is the task and function of the motor nerves to lead towards the periphery the impulses sent out by their ganglion cells in the spinal cord, their activity always admits of ready perception through the muscular twitching. Even when the nerve is divided and artificially excited at the peripheral end, the muscles betray it. On the other hand, no visible physiological reaction is found at the central origin of the motor fibre when stimulated at the periphery, so that at first we were quite in darkness as to whether in general it conducted centripetally. Nature, however, has presented us with a contrivance by which we are enabled to demonstrate the possibility of such an inverted or centripetal nerve-conduction. The contrivance consists in the branching division of nerve-fibres, so frequently found in muscles, as will at once be seen in a preparation from a frog (Fig. 3). In many muscles these branchings are so arranged that we can use them for an experiment as simple as it is conclusive of nerve-conduction in both directions.

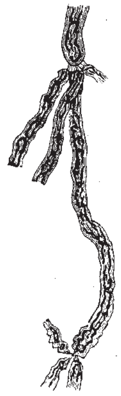

FIG. 3.

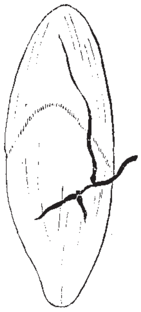

FIG. 4.

In the *gracilis* muscle of the frog the nervation is fashioned in the manner displayed schematically upon this diagram (Fig. 4), and in more detail on the following (Fig. 5). In reality the arrangement is like this. Now, if I cut up the muscle according to this diagram (Fig. 6), we get at the tip, *z*, nerve-fibres, which are connected with the muscle-fibres at *o* and *u* only by the branchings at the points *x x*, but which in life served only for the parts of the muscle removed at *f* and *f'*.

An experiment[2] will now convince you that nerves severed from their own muscle-fibres act quite well backwards upon those placed centripetally to them, which they can only do if nerves can also conduct centripetally, and so long as a path is preserved for this through the branchings. If we cut out the neighbourhood of the branchings, it is all over with the reaction of the muscle.

We can make another experiment on the same muscle.[1] We see that when we excite the lower tip of the muscle, only the lower portion twitches and not the upper. The two portions are in fact connected only by means of a very short tendon, the so-called *inscription*, which passes completely through the muscle (*i i* in Fig. 5), so that it really consists of two muscles. If the nerve common to both is stimulated at any point, then both parts of the muscle contract, but if the muscle substance itself is stimulated, then the contraction travels no further from the place where the stimulus was applied than to the limits of continuity of the muscle-fibres.

The power of motor nerves to conduct in both directions is certainly of general significance in regard to the inner mechanism of nerves, but we have only approached it here, because it was necessary for the decisive proof of muscular irritability, as obtained in our last experiment with the *m. gracilis*. Whenever a muscle is provided with a nervation and branchings of the separate nerve-fibres like that of the *gracilis*, some group of muscle-fibres can serve to indicate whether a stimulus has affected this alone or the nerves lying in it as well. If nerves are present at the point of stimulation, and if the agent was at the same time a nerve stimulus, this is shown by the simultaneous contraction of distant parts which are accessible by means of the nerve's power of conducting in both directions. In cases where we can see the coarser nervation, the indirectly produced contractions can be predicted, and these form so certain a criterion of neuro-muscular excitations that by them the presence of the finest nerves may be proved, whose existence might otherwise be quite incapable of proof by any other means, as, for instance, by the use of the microscope. If these contractions are wanting, as was the case in our experiments with the lower end of the muscle, we know that either the spot stimulated is free from nerves, or that the stimulus employed was ineffectual as to the nerves, and affected the muscle substance exclusively. In both cases, then, independent irritability is proved for those muscle-fibres which were directly excited and contracted.

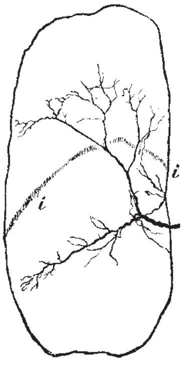

FIG. 5.

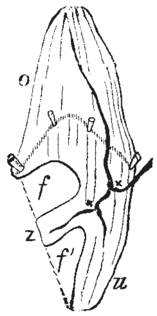

FIG. 6.

Now, since we have just employed an electric stimulus which is equally effectual on muscle and nerve, it follows that we had to do with the first case; that is to say, the muscle showed itself free from nerve at its end. We have reason for specially bringing forward this experimental proof of the absence of any kind of nerves in large tracts of muscle, because it compels those who in spite of all assume the presence of nervous matter in certain microscopic disks and striæ of the muscle-fibre as a whole, to deny that this supposed nervous element possesses any power of conducting in both directions or any irritability at all; for in fact it is not possible to excite the motor nerve of a muscle-fibre by any stimulus whatever applied to the actual terminations of the nerve within the fibre. The facts besides combine to prove, as need hardly be said, yet another proposition—they prove at the same time that pure muscular excitation does not travel back to the nerves.

This may be shown still better with the small pectoral muscles of the frog's skin than with the *m. gracilis*. We need only dissect it in the manner shown in the drawing (Fig. 7), and stimulate the spots *n* and M: if we stimulate *n*, everything contracts; if M, the excited half only.

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[1] "On the Origin and the Causation of Vital Movement (*Ueber die Entstehung der vitalen Bewegung*)," being the Croonian Lecture delivered in the Theatre of the Royal Institution on May 28, 1888, by Dr. W. Kühne, Professor of Physiology in the University of Heidelberg. Continued from vol. xxxviii. p. 629.

[2] Kühne, "Ueber das doppelsinnige Leitungsvermögen der Nerven." *Zeitschr. f. Biol.*, vol. xxii. p. 305. To demonstrate the experiment on the *gracilis*, the muscle was fixed on a white piece of cork by needles, and held by elastic holders, and its image thrown on the wall highly magnified by the so-called Krüss lantern.

[1] Kühne, *ibid.*, pp. 312, 324.

The preparation which you now see (corresponding to Fig. 2), and which shows the nervation of the very thin muscle with all the nerve-endings stained dark with gold, makes that relation clear, for here again in truth the result of morphological research is in gratifying accordance with results obtained experimentally. The muscle is seen to be for the most part free from nerves; indeed the entire nervation with all the nerve-endings might be said to be formed of one nerve-*line* only, if we disregard the few digressing fibres, which, again, in part are not motor.

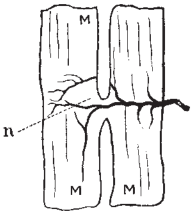

Fig. 7.

Under rather higher powers we see the nerve-endings proper (Fig. 8), the distinct demonstration of which by means of the gold method has now been achieved, in much the same way as here, in all the classes of vertebrates with the exception of the osseous fishes. In all cases these decisive preparations have proved that the vastly preponderant number of the muscle-fibres is entirely free of nerves, and that the nerve-endings are confined to very small spots which we term fields of innervation. Most muscle-fibres have only one field of innervation, very long ones occasionally several, at the most eight. Thus the assumption, opposed to the idea of independent irritability, that muscle-substance is well nigh completely riddled with nerves, is refuted and rejected from the morphological side also.

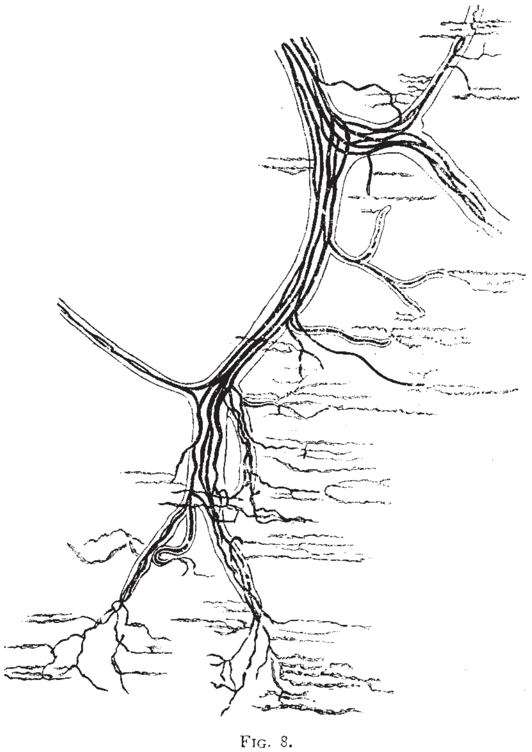

Fig. 8.

From the absence of nerves in long tracts of muscle-fibre we immediately conclude that the latter shares with nerves the faculty of independently propagating its own excitation. This is what the beautiful microscopic observations of Sir William Bowman[1] on insects' muscles long since led us to suspect. As in the nerve, so in the muscle, conduction takes place in every direction, and as the field of innervation almost without exception occupies a median position during a normal contraction, the conduction takes place in both directions, towards the tendinous ends. By way of distinction the velocity of conduction is, according to species, temperature, &c., three to ten times less than von Helmholtz fixed it for nerve. As conduction in irritable tissues means nothing else than that one excited spot becomes the stimulus for the adjoining portion at rest, the independent irritability of the muscle-fibre comes into employment in every movement and during the entire duration of life; from the moment that the field of innervation becomes active all the muscle substance remains left to itself, and until the contraction is ended must be regarded as independent and acting in response to its own direct excitation.

Once clear on the fundamental question, and sure as to the method we have to employ in order to stimulate according to choice either muscle- or nerve-substance alone, or both together, we may seek to determine in what respect the irritability of the two components of the motor machine differs. The differences as regards chemical stimulation appear very great; in respect of electric, thermic, and mechanical, on the other hand, only quantitative. However, under chemical stimulation, according to Hering's classical researches,[2] a point formerly overlooked comes into consideration—namely, the complication introduced by the electromotive behaviour of the tissue, an automatic electrical stimulation one might say. When stimulation takes place by moistening the transverse section with conducting liquids, it is indeed difficult, if not impossible, to trace the chemical factor in presence of the electrical. Gaseous stimuli alone, like ammonia, have thus far remained free from the suspicion of acting electrically. To these a few others of similar action, such as bisulphide of carbon,[3] have been added, and such as are conveyed to the muscle by blood-vessels, and bathe the fibres from all sides. With these in particular we may class distilled water, which is excessively destructive to irritable substances, von Wittich[4] being the first who showed how strongly it stimulates muscles, while killing nerves without excitation. But, again, with this kind of stimuli, we cannot at present tell whether they do not set up in the tissues, over narrow but numerous areas, excitatory electric currents, thus working only indirectly by way of auto-electric stimulation. And since, finally, the same might apply to the thermic and mechanical actions which likewise arouse demarcation currents in the muscle—that is, to all stimuli—we find ourselves in the presence of the possibility of reducing all irritability to a reaction to electrical processes, and of seeing vital electricity elevated into immeasurable importance.

The means by which muscle may be stimulated interests us, in the first place, on this account—to ascertain, once for all, how it procures its excitation *in life*, or what may be the action of nerve upon it. Did we know that, we should have grasped at the same time the nature of nervous activity.

Nerves end blindly in the muscles; as a rule they are not even finely pointed, and still less do they spread out diffusely in such a way as might make the true ending difficult to find. They end quite distinctly. But the ends always lie beneath the sarcolemma, in such a way that no foreign tissue intrudes between them and the muscle, so that what is fluid in the muscle can directly moisten the nerve. The sublemmar nerve is clothed with nothing else than the axilemma. The nerve never penetrates into the depths of the muscle-substance; on the contrary, it remains confined to the sublemmar surface of the contractile cylinder or prism. Each nerve-end consists of several branches, like antlers, arising by division, which together form the terminal nerve-branch. Apart from the form of the antlers, this short description is exhaustive for many animals, since neither in the sublemmar nerve need any special additional structures occur, such as nuclei, nor any kind of modification of the muscle-substance in the field of innervation. There is much to indicate that the nerve-fibre proper, or axis-cylinder, does not change its constitution in passing through the sarcolemma, still it is to be remarked that the twigs of the terminal branches, although as long as they live often apparently longitudinally striated, have not yet, even in the most favourable stainings, been found to present the general fibrillar structure of nerves.

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[1] "On the Minute Structure and Movements of Voluntary Muscle," Phil. Trans., 1840, p. 457; and "Muscle—Muscular Motion," in the "Cyclopædia of Anatomy and Physiology," edited by B. B. Todd, vol. iii. 1847, pp. 506–530.

[2] "Ueber direkte Muskelreizung durch den Muskelstrom," Vienna, *Sitzber. k. Akad.*, vol. lxxix., Abth. 3, 1879.

[3] "Ueber chemische Reizungen; nach Versuchen von. stud med. C. Iani." *Untersuch. aus der Physiol. Instit. der Univ. Heidelberg*, vol. iv. 1882, p. 266.

[4] "Experimenta quaedam ad Halleri doctrinam de musculorum irritabilitate probandam instituta," Königsberg, 1857; and *Virchow Archiv*, vol. xiii. 1858, p. 421. In these papers, with the discovery of the excitation of muscle by distilled water, appears without doubt the first fact which overthrew the old theory of the equal irritability of muscle and nerve.

According to these results of morphological research, it appears that contact of the muscle-substance with the non-medullated nerve suffices to allow the transfer of the excitation from the latter to the former. The only strange thing is that in reversed order excitation of the muscle never extends to its own nerve. This is still stranger because, according to Matteucci's well-known discovery, a *foreign* medullated nerve simply laid upon the muscle is powerfully excited by the contraction—so powerfully that the smallest contracting muscle barely touching it in more than a mere point excites the strongest nerve, while, on the other hand, we never see muscles excited by nerves which are merely pressed against them.

In the investments, then, of the nerve and the muscle-substance appears to exist one of the elements which admits the neuro-muscular excitation *exclusively* to the field of innervation, and among those investments it need not be the medullary sheath. The delicate membranes of the sarcolemma and neurilemma suffice, for muscle cannot be excited by superimposed *non*-medullated nerves. At any rate, I have tried in vain to excite muscles by the most intimate contact of the fine terminal ramification of the optic nerve in the retina or the *n. olfactorius* from the pike, or even the delicate nerves of Anodonta, by stimulating these non-medullated nerves.

If we imagine the activity of the nerve to start with a chemical process, and that a chemical stimulant, as du Bois-Reymond[1] once suggested, is, at the same time, secreted in contact with the muscle, we understand very well the necessity of direct contact, and in this case it would suffice if the sublemmar nerve were to run in *any* form for a short distance under the sarcolemma. The branching then would mean the enlarging of the contact. But however rich and intricate the ramifications may be, we can by no means say they display throughout the principle of increase of superficies; on the contrary, they are often astonishingly poor and small. As concerns their form, they are *not* irregular, but so strikingly uniform that this point deserves particular attention as being apparently indispensable for innervation.

Instead of describing the forms, allow me to show you the object itself in a selection taken from the most diverse vertebrates. First from the Amphibia (Fig. 9): rod-like branchings with long outstretched twigs, a form which crops up again in a remarkable way in many birds. The rule here is asymmetry of the divisions: all the twigs have the form of a bayonet.

The following preparation shows the termination in the dog (Fig. 10). Here the branches are crooked, and hence quite divergent, so that the points of agreement with the form of the Amphibia are at first overlooked. But if we examine the divisions, you will remark that these are again unsymmetrical and give off branches whose ends lie very diversely removed from the common place of origin. The ends are, as a rule, turned towards each other, and often so approximated that it is at times troublesome to find the gaps between them, and if they do not lie in the same plane they appear to be united into a ring. In other cases one end overlaps the other, but we then find that all the points of the branches which are turned towards each other lie at unequal distances from the nearest bifurcation. This law holds good in all the thousand cases of motor endings thus far observed, and shows a strict order in the apparent chaos of these structures. And yet among the organic forms there is scarcely one which varies so much in other respects, and often is so inextricably complicated as this.

The drawings (Fig. 11, from the muscles of the guinea-pig, and Fig. 12, of the rat) and a preparation from a lizard (Fig. 13) may serve as a voucher for the truth of the above statement. We see there everywhere the hooks making their appearance with a short and a long claw, like the swivel we hang our watch on in the pocket.

The voluntary muscles of all vertebrates and of many invertebrates consist of fibres, the contents of which are pefectly regularly disposed in layers and transversely striped. For shortness, this striped mass may be called "rhabdia." This it is which has been universally identified with the contractile substance. But it has been ascertained that in many cases the nerve-ending does not come at all into direct contact with the rhabdia, but with another mass, which is highly nucleated and of pap-like softness. This latter is unstriped, and has all the appearance of protoplasm. It occurs in very varying quantity under the nerve-antler; in Amphibia, where the sublemmar nerves run out in a long course, it is not apparent as a separate layer, but it occurs more abundantly in the same measure that the branchings retract, and the field of innervation becomes smaller. At first it is found chiefly between the twigs, in the intervals of the branching, and then in the form of a sole, which, among the much-contorted branchings of reptiles and mammals, grows thicker, till it sometimes in some nerve-eminences forms quite a thick cushion. Since we have succeeded in making the nerve-endings visible in uninterrupted series of very fine sections of mammalian muscle stained with gold, there can no longer be any doubt that the complete separation of the sublemmar nerves from the rhabdia by measurable layers of sole-protoplasm, though not the rule, is yet by no means rare, and that many muscles possess no other sort of nerve-endings than such as these with apparently indirect contact.[1]

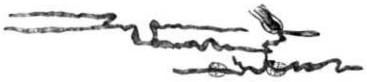

FIG. 9.

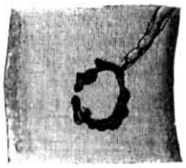

FIG. 10.

It would be difficult to understand why the innervation should have in some muscles, as in the Amphibia, no intermediate layer, while having in the majority of cases an interrupted layer, and in others a continuous layer of varying thickness to traverse. But when we consider what the substance of the sole is, of what it consists, how it is distributed, and when we know its origin, it appears that it is identical and stands in continuous connection with the long-known second constituent of muscle-fibres, of which, as well as of the rhabdia, the fibres are composed. It is that substance, considered by Max Schultze to be the protoplasmic remnant of the cells composing muscle, which occurs in greatest amount around the nuclei of muscle, and extends in long threads throughout the entire muscle-fibre. So many transverse connections occur on the very numerous stronger and finer nucleated threads that the whole mass, called sarcoglia, becomes a trellis-work almost of the same fineness as the better-known transverse striation of the rhabdia, and everywhere surrounds and interpenetrates the latter. This minute internal structure of muscle has only become at all well known since the introduction of gold staining, thanks especially to Messrs. Retzius and Rollett.[2] Had it been suspected earlier, and had we appre-

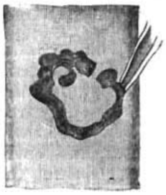

FIG. 11.

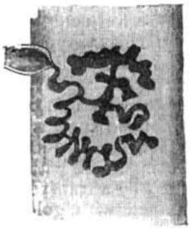

FIG. 12.

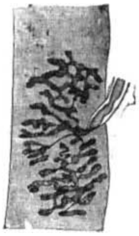

FIG. 13.

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[1] "Gesammelte Abhandlungen zur allgemeinen Muskel und Nervenphysiologie," vol. ii., p. 700.

[1] Kühne, *Verhandlungen des Naturhist.-medicinischen Vereins zu Heidelberg*, Neue Folge, vol. iv. pp. 4, 5.

[2] G. Retzius, "Biologische Untersuchungen," 1881. A. Rollett, "Untersuchungen über das Bau der quergestreiften Muskelfaser," *Wien. Akad. Denkschr.*, vol. xlix., 1885.

ciated the volume of the sarcoglia, whose existence is thereby shown, and which rivals that of the rhabdia, we might have studied this component of muscle in its physiological relations to contractility, as well as in its morphological and genetic relations, which are the only ones yet known.

If now, in many cases, it appears that the nerve comes in contact only with the surface of a thick layer of sarcoglia, while the rhabdia everywhere is covered by very fine layers of the latter, whose absolute absence in the field of innervation can nowhere be demonstrated, we have to conclude that in general the nerve does not act directly upon the rhabdia, but only on the sarcoglia. This at once gives the latter a physiological interest. We have to ask whether the glia is the medium that conducts the stimulus between nerve and rhabdia, or whether it is itself the contractile element, while the rhabdia has a signification other than that formerly attributed to it when we were completely ignorant of the glia.

All contractile substance requires the co-operation of an elastic element. Where is this to be found in the muscle-fibre? The envelope of the sarcolemma, which is certainly elastic, but delicate, and whose mass is almost infinitesimal compared with that of the muscle-fibre, cannot satisfy the requirement; but more solid structures freely distributed in the paste-like sarcoglia could perhaps do so, and such we find in the rhabdia, in the form of prismatic particles, ranged with such constancy and with such regularity longitudinally and transversely, that we may hold them to be the elastic element. Then the sarcoglia would become the contractile element, and the nerve would have an easier task.

I could wish that this view might be accepted as an hypothesis. As far as I can see, it does not contradict experience, for it only puts back the muscle nearer to the protoplasm and to all that is contractile, and so far coincides with experience that we find muscles in the same measure less elastic and more sluggish in protoplasmic movement the richer they are in sarcoglia, as in the case of the red muscles, nucleated and rich in glia, which contract more slowly but with greater power than the white muscles, poorer in glia, which are quick and spring-like, and also the sluggish embryo muscles, in which glia predominates because as yet but little protoplasm has been converted into rhabdia; and further the cells of unstriped muscle-fibre, which are wanting in the regular transverse striation, and contain, as it appears, besides more abundant glia, an elastic material of special form and arrangement.

The hypothesis would be overthrown if contractile fibrils were found in which no sarcoglia was to be detected. But even in the finest fibrils of *Stentor*, the structure of which Bütschli[1] has recently elucidated, we must hold the significance of punctated transversely penetrating indentations to be protoplasmic, and we can therefore scarcely expect ever to find a contractile thread in which nothing whatever should be found of the primitive contractile material such as it everywhere exists.

Of late, this view[2] has been defended from the purely morphological side,[3] on the strength, namely, of the very fine reticular structure of protoplasm to which more attention is being paid, and which is demonstrable on objects of all grades of organization. Protoplasm, in fact, is not so formless as at first appeared, but shows a structure comparable with nothing better than with the appearance presented by a transverse section of muscle with its glia framework stained with gold. We may expect that these reticular structures, whose consistency appears to vary extraordinarily, will some day lead to the establishment of a fruitful hypothesis of the inner mechanism of protoplasmic movement, in place of that held hitherto, which affords no glimpse into the essence of vital mechanical work.

Compared with this larger problem, that of the causation of vital movement appears the more accessible of the two, the latter being considered as a physiological inquiry after the constitution of the normal stimulus by which work is done. Perhaps, indeed, the answer is to be looked for from the most perfected organization of muscle, where the iniatory process is localized by a distinct nerve-ending, rather than from the primitive organization, where the excitation may set in at any place, and lies in the protoplasm itself. We know distinctly that the muscle-wave begins in the field of innervation, for we have long seen the natural contraction in the interior of transparent insect larvæ starting from the nerve-eminences. We know this also from the experiments of Aeby, who followed the muscle-wave myographically from the nerve-line onward, and now we are able to display the beginnings of the contraction as local thickenings at the point of attachment of the nerves caught and fixed by sudden hardening. Since the nerve grasps the muscle in a restricted region, it expends its action upon this exclusively; that which follows on as muscular activity is the nerve's work no longer.

Galvani and his successors for more than a century suspected that nervous forces were electrical, and, in reality, the celebrated champion of electro-physiology in our day has been able with the galvanometer to render the excitation of nerves, unattached to muscles or ganglion-cells, evident as the negative variation of the natural nerve-current, to cause movement of a magnetic needle instead of a muscle, or to put the needle in the place of sensation. After this no consideration of the nature of nervous activity is conceivable which does not take into consideration this discovery of du Bois-Reymond's—least of all where the nerve has to excite something with which it is not fused, like muscle, but which it only touches, and that not directly, while still invested by the axolemma. Only during excitation, as Ludimar Hermann has taught us, are electric currents issuing from the nerve through its conducting surroundings, in which the course of these currents of action is to be estimated from the duration of the negativity of the nerve-tract excited, and from the speed of propagation of the nerve-wave, if we know the conductor and the disposition of the nerve. The motor ending fixes the latter, and so peculiarly that we can only presuppose from it a furthering of the excitor effects of the currents of action.

The currents of action of muscle, whose electromotive behaviour agrees so wonderfully with that of nerve, have long been proved to produce excitor effects, although only powerful enough to act upon nerves; but there are also, under certain conditions discovered by Hering, such effects from nerve to nerve.[1] Is the possibility, we may hence ask, to be excluded, of one muscle exciting another, and is it quite impossible that a nerve only throws a muscle into contraction by means of its currents of action?

The first question we can answer. I will do so by a simple experiment. Two muscles, the nerves of which are disposed of by poisoning with curare, need only to be pressed together transversely over a narrow area to make a single muscle of them of double length, in which the stimulation and contraction are propagated from one end to the other. Since the transference from one muscle to the other is done away with as soon as we bring the finest gutta-percha between the muscles as an insulator, or gold-leaf as a secondary circuit, the first muscle must have excited the second electrically.[2]

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[1] "Dr. H. G. Bronn's Classen und Ordnungen des Thierreiches," neu bearbeitet von O. Bütschli, Leipzig und Heidelberg, 1888, vol. i. p. 1298.

[2] Kühne, "Neue Untersuchungen über motorische Nervenendigung," *Zeitschr. Biol.*, vol. xxiii. pp. 88-95.

[3] A. van Gehuchten, "Étude sur la structure intime de la cellule musculaire striée," *La Cellule*, vol. ii. p. 289.

[1] *Sitzber. der k. Akad. zu Wien*, vol. lxxxv. Abth. 3, 1882, p. 237.

[2] Kühne, "Secundäre Erregung vom Muskel zum Muskel," *Zeitschr. Biol.*, vol. xxiv. p. 383.

THE ASTRONOMICAL OBSERVATORY OF PEKIN.

IN the course of a lecture delivered before the Pekin Literary Society, on the Astronomical Observatory of the Chinese capital, Prof. Russell said that it is the oldest in the world. The oldest in Europe is that of Denmark, founded in 1576 by Frederick III., at which Tycho Brahé made his famous observations. The Royal Observatory at Paris was not opened till 1671, and that of Greenwich three years later. The Pekin Observatory was established in 1279, in the reign of Kublai Khan, the first emperor of the Mongol dynasty, and three of the original instruments yet remain. In 1378, these instruments were probably used in observing Halley's comet, and they will be used twenty-two years hence to witness its next return. If the visitor enters by a door in the south wall of the Observatory, he comes into a court running east and west. In this court are kept the three original instruments. There were four at one time, but the fourth, a celestial globe, has disappeared. Kuo Shouching, a Chinese astronomer, who flourished in the reign of Kublai Khan, was the maker of these. Before their construction, bronze astronomical instruments, which were made about the year 1050, were used, first at K'ai Fêng Fu, the capital of Honan, whence they were removed to Pekin. Kuo Shouching found these